The public, the protester, and the bill: do legislative agendas respond to public opinion signals?

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Abstract

Legislators adapt their policies and agendas to public priorities. Yet research on dynamic representation usually focuses on the influence of public opinion through surveys leaving out other public opinion signals. We incorporate mobilization of the public through protest. Combining insights from social movement studies and political science, we expect protest not to have a direct effect on attention change in legislative agendas. If anything protest should have an amplification effect on public priorities. Using a new and unique data set covering collective action, public opinion and legislative agendas across almost 40 years in four Western democracies, we confirm the effect of public opinion through surveys but find no support for a direct effect of protest. Protest rarely moves legislators: only in very specific issues will protest interact with public priorities and affect attention change in legislative agendas. Our results have important implications for policy representation.

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1 Introduction

Responsiveness of policy makers to the issue priorities of the public is an important aspect of democratic representation (e.g., Bevan and Jennings 2014). Yet, since attention is scarce, policy makers frequently cannot attend to all incoming information by their citizens (e.g., Jones and Baumgartner 2005). This leads policy makers to focus their attention on issues citizens care most about while ignoring others the public is less concerned with (e.g., Mortensen et al. 2011). In political science, an impressive body of research has put these basic ideas of democratic representation to test and confirms that policy makers tend to respond on issues that are important to the public (Miller and Stokes 1963; Page and Shapiro 1983, 1992; Burstein 2003; Gilens 2005; Jones and Baumgartner 2005; Bartels 2008; Hobolt and Klemmensen 2008; Soroka and Wlezien 2010; John et al. 2011; Mortensen et al. 2011; Bevan and Jennings 2014; Burstein 2014).

Far less attention, however, has been paid to different types of opinion signals and how they might affect, thwart or reinforce each other. Although numerous accounts show that politicians attend to a variety of sources when trying to figure out what the public cares about – for instance, from mass media (Herbst 1998), over contact with constituents (Fenno 1978), opinion polling (Geer 1996) to different forms of advocacy (Burstein 2014) – this variety of signals has not been adequately reflected in the empirical literature on democratic representation. Our study addresses this research gap. Besides public opinion through surveys, we add one other expression of public opinion to the puzzle: protest. We ask: Do policy makers react to issues primed by protest? And, how does the influence of protest compare to the cues presented by public opinion in the polls?

Studying responsiveness to protest and how protest and public opinion signals relate to each other is important for a number of reasons. First, in the last few decades, mobilization of public opinion through protest has increased across Western democracies (Dalton 2017), challenging the role of political parties as traditional channels of representation. Social movement scholars and comparativists speak of "demonstration democracies" and "social movement societies" (Meyer and Tarrow 1998; Norris et al. 2005; Dalton 2008). From a democratic theory perspective, thus, this rise in protest brings about the question of whether governments should be responsive to this other, mobilized form of public opinion.

Second, this rise in protest has renewed attention to the political consequences of protest as well, although primarily in sociology (for reviews, see: Giugni 1998; Amenta

et al. 2010). Although comparative scholarship has identified an agenda-setting effect of protest – i.e. when protest activity relating to an issue increases, political elites start to devote more attention to that issue (Vliegenthart et al. 2016: 838) – contrary to the political science research on democratic linkage, studies on the impact of protest have produced mixed findings (Giugni 2007; Uba 2009; Amenta et al. 2010). One reason for these inconclusiveness is that the lion's share of protest impact studies are case studies, focusing on one movement or issue in a single country, giving researchers little leverage to tease out potential contingencies (for notable exceptions, see: Walgrave and Vliegenthart 2012; Vliegenthart et al. 2016; Hutter and Vliegenthart 2016).

Third, how the impact of protest relates to alternative expressions of public opinion known to determine policy making – like opinion polls – has so far received only scant empirical attention (Burstein 2003). Work by Agnone (2007) and Giugni (2007) point towards an interaction effect of protest and public opinion. While Agnone proposes an "amplification model", whereby the impact of public opinion on legislative action is greater depending on the level of protest, Giugni argues for a "joint-effect model", where movement impact on policy change is forthcoming when public opinion intervenes together with movement mobilization. However, such ideas have only been tested on a limited number of countries or issues.

With this study, we seek to contribute to both political science and sociology. We expand the limited comparative studies of dynamic agenda representation by adding protest to the mix of incoming signals; and we contribute to the literature on protest impact by scrutinizing multiple issues across multiple countries. To date, we are unaware of comparative studies that jointly consider the impact of public opinion through surveys and public opinion through protest on policy makers' legislative agendas.

We hypothesize that public priorities, as expressed through surveys, have a stronger impact on legislative agendas compared to public priorities as expressed through protest. The main signaling function of protest, we suggest, lies in sensitizing legislators to certain issues that are already quite dominant across the general public, as such amplifying the impact of general public opinion signals (Agnone 2007; Giugni 2007).

Based on a novel assembled dataset, we present a series of error correction models of time-series cross-sectional data on legislative agendas collected by the Comparative Agendas Project, public issue priorities from a variety of national opinion polls, and protest from three different data sources across almost 40 years (1974-2011) in Germany, Spain, the United Kingdom, and the United States. Our study is the first that combines data on public opinion polls, protest and legislative agendas across countries and for

such a long time span.

Our pooled analysis confirms established findings on long-term effects of public issue priorities on policy agendas and reveals that protest does not have any impact on change in legislative agendas. This general pattern is largely supported in issue-specific analyses, whereby protest only has a significant effect on one issue – social welfare. However, we find support for an amplification mechanism in cases of domestic policy that touch citizens' life more directly like education, housing and unemployment.

On the one hand, we find that protest does not play much of an important role in changing policy makers' attention in legislative agendas, legitimizing political scientists' ignorance of this democratic input signal allegedly on the rise. On the other hand, our findings do suggest that protest *can* be an influential informational resource for policy-making. The impact of protest, however, is highly contingent and only rarely materializes. These findings come on top of robust evidence for a positive effect of public priorities on issue attention in legislative agendas, straightforwardly confirming previous accounts of dynamic agenda representation. In sum, Western policy makers tend to adapt their legislative agendas to the public at large while frequently ignoring the priorities signaled by protesters. Only if protesters' signal is strong and supported by public priorities will protest matter for attention changes.

Our findings have important implication for mass-elite linkages and social movement strategies. Our finding that policy makers follow signals from the public at large but not to specific groups in the society confirms that democratic governments tend to represent and respond to the concerns of the general public rather than to the priorities of a particularly active segment of the public. Our finding is thus reassuring for those who see demonstration democracies as a threat to the representation of the silent majority in favor of the representation of a loud minority. On the other side, the very limited influence of protest may be disappointing for those who, instead, hoped for a a more profound influence on agenda dynamics of more engaged parts of the public.

Our result that protest has a larger influence on issue attention in legislation when the priorities of protesters are strong and aligned to the priorities of the general public, even if for a limited number of issues, is important for social movement strategies and confirms previous findings of a limited impact of protest on policy (e.g., Burstein and Linton 2002; Giugni 2007). Our findings are to some extent in line with Agnone's (2007) amplification mechanism and suggest that the context in which protest takes place is crucial in determining the power of protest itself. The protest's role in influencing policy makers' attention would be more successful if understood as a salience-raising element

rather than an independent signal.

2 How legislative agendas adapt to external stimuli

Political science research on the opinion-policy nexus originated from constituent-legislator dyadic representation (e.g., Miller and Stokes 1963), developed in collective representation (e.g., Weissberg 1978) and, with the incorporation of time, evolved in dynamic representation (e.g., Stimson et al. 1995). The common denominator of the incredible amount of studies on the opinion-policy relationship is that public opinion has been essentially studied as a monolithic entity, namely the mean voter, measured through public opinion polls (for an overview, see: Manza and Cook 2002).

In general, research suggests that public opinion sends two signals to policy makers. First, the public reveals its opinion through expressing their policy preferences – e.g., asking for more or less spending (Wlezien 1995; Soroka and Wlezien 2010). Second, the public also signals the importance or priority of policy issues – e.g., by naming the most important issue facing the nation (Wlezien 2005; Jennings and Wlezien 2016). This duality has generated two strands of research. The first perspective looks at responsiveness in terms of *position* and investigates whether citizens' preferences have an impact on policy outputs (e.g., Wlezien 1995; Lax and Phillips 2009; Soroka and Wlezien 2010; Lax and Phillips 2012). The second perspective looks at responsiveness in terms of *attention* and investigates whether policy makers adapt their agendas to citizens' issue priorities (e.g., Jones and Baumgartner 2005; John et al. 2011; Mortensen et al. 2011; Bevan and Jennings 2014).

Importantly, governments first need to pay attention to public issue priorities for policy responsiveness to preferences can happen (Jones and Baumgartner 2005). The former is a precondition for the latter. Hence, in this paper, we analyze whether attention change in legislative agendas responds to changes in issue priorities of the public. We know from previous research that different policy issues promote different levels of policy representation – e.g., responsiveness on domestic issues is found to be higher than on foreign policy issues (Hobolt and Klemmensen 2005, 2008) – and that the latter also depends on issue salience. If dynamic agenda representation works, then when an issue becomes salient to the public, it is more likely that the government will respond on that issue by paying more attention to it in its agendas. For our purposes, this implies that a change in public opinion's priorities would be followed by a change of government

priorities in its legislative agenda.[1]

Two mechanisms are understood to drive legislators' responsiveness on salient issues: policy makers' desire of being re-elected and attention scarcity. On the one hand, salient issues should be decisive at the ballot box. Since legislators have a tremendous interest in seeking re-election they cannot afford to neglect voters' concerns and demands (Downs 1957; Strøm 1990; Stimson et al. 1995), and this also applies to citizens' issue priorities. On the other hand, given the complexity and the amount of public demands, attention is scarce and this has severe consequences for agenda representation (Kingdon 1995; Jones and Baumgartner 2004; Jennings and John 2009; Bevan and Jennings 2014). Hence, policy makers prioritize those issues on their agendas which are most important to the public and pay less attention to the ones the public is less concerned with (Mortensen et al. 2011).

We note that most of previous work on agenda responsiveness (and congruence) focuses on rhetorical agendas and research based on legislative agendas is mostly made of single-country studies (Jones and Baumgartner 2004; Chaqués Bonafont and Palau 2011; Lindeboom 2012; Brunner 2013; Visconti 2016). Thus, we believe that a comparative test on the effect of public priorities on change in legislative agendas is still important, not only in comparison with protest priorities but also for enlarging existing empirical evidence. In summary, we agree with previous research and suggest that policy makers adapt their legislative priorities to public priorities:

- **Public Opinion Hypothesis**: Public issue priorities have an effect on attention change in legislative agendas.

Whereas evidence for the translation of public issue priorities to policy priorities is robust, there is less consensus regarding the role of political protest in democratic linkage processes (for a recent overview see: Amenta et al. 2010). For long, the power of protest to shape the legislative agenda has been left empirically unexplored. Whereas sociologists presumed social movement activity to be an important force of social change and, therefore, studied processes of protest emergence and mobilization (McAdam 1982), political scientists regarded protesters as "beggars at the policy gates", not even considering protest when studying democratic linkage.

[1] Of course, law-making process is not always motivated by changes in public priorities. For instance, institutional friction matters (e.g., Jones et al. 2009; Bevan and Jennings 2014). Further, while some research suggests that governing parties tend to also focus on the issues they own in their legislative agendas (e.g., Egan 2013; Green and Jennings 2019), government partisanship seems to matter less (e.g., Bevan and Jennings 2014) compared to compulsory issues or pressing problems that demand legislative action (e.g., Adler and Wilkerson 2012).

Since the turn of the century, however, the legislative impact of protest has become increasingly empirically scrutinized, albeit primarily by sociologists (Amenta et al. 2005; Andrews 2001; Biggs and Andrews 2015; Giugni 1998; Soule et al. 1999; Walgrave and Vliegenthart 2012), less so by political scientists (for notable exceptions, see: Costain and Majstorovic 1994; Gillion 2013). Moreover, most of these studies tend to be case studies, focusing on a single movement or issue, hindering generalization (for multiple issue studies, see: Hutter and Vliegenthart 2016; Vliegenthart et al. 2016). The standing conclusion of this literature is that protest can matter, but that it does so rarely independently or directly. Rather, the impact of protest is contingent upon the context in which it is organized. In the next few paragraphs we elaborate on this argument from a policy makers' perspective.

Why would policy makers be responsive to protest signals? Skeptics hold that there is little incentive for elected officials to respond to protest. As gaining re-election is the main goal for elected officials, their actions should especially be guided by what occupies the majority of the public (Arnold 1990). So, only when protest succeeds to make a powerful assertion of popular sovereignty, it might directly influence policy makers (Wouters and Walgrave 2017). Most often, however, protest is staged by disenfranchised actors with more extreme, minoritarian stances and select, deviating priorities compared to the median voter (Lohmann 1993). Giugni (2007: 54) underscores the limited signaling strength of protest when he describes movements as "minority actors that have little power". Also Burstein and Linton (2002) hold that the potential political impact of protest is probably only moderate at best, and likely to decline or even disappear when measures of public opinion are taken into account. Amenta (2014), finally, goes even a step further and posits that protests are often counterproductive because protesters frequently mobilize in response to threatening political circumstances.

Two empirical findings further ground the claim of no direct link between protest and legislation. First, several studies find that especially organizational capacity and institutional strategies of movements (for instance lawsuits and other legal actions, petitioning, letter-writing, lobbying, press conferences) matter in shaping policy outcomes, far less so the extra-institutional strategy of protest (Johnson et al. 2010; Olzak and Soule 2009; Soule and Olzak 2004). Second, research suggests that the influence of protest is most likely at the early agenda-setting stage and decreases dramatically along the policy cycle (King et al. 2007; McAdam and Su 2002). In sum, there are reasons to expect that protest has no direct effect on attention change in legislative agendas, or at best weaker compared to that of public opinion through polls. Here, for the first time,

we formally test such a hypothesis across a multitude of issues and countries:

- **Protest Hypothesis**: Protest does not have a direct effect on attention change in legislative agendas.

Social movement scholars have developed more comprehensive frameworks to account for the impact of protest, however. In line with the dominant political opportunity perspective and the political mediation model (Amenta et al. 2005; Kriesi et al. 1995b; Meyer 2004), these studies hold that especially a favorable context is decisive for movement success (Amenta et al. 2005, 1992; Cress and Snow 2000; Giugni 2007; Johnson et al. 2010; Olzak and Soule 2009; Soule and Olzak 2004; Vliegenthart et al. 2016). Without supportive external resources presented by a favorable context, protest is toothless; given the right circumstances, however, protest can make a difference. For instance, the presence of political allies and the composition of governments (Amenta et al. 2010; Burstein et al. 1995; Cress and Snow 2000; Lipsky 1968; Vliegenthart et al. 2016) or favorable media attention (Vliegenthart et al. 2016; Walgrave and Vliegenthart 2012) is expected to boost protest power.

Here, we focus on one other key characteristic of the political context: the issue priorities of the general public. We expect the impact of public opinion on legislative agendas to increase when also protest activity on that particular issue increases. Our argument is straightforward. Protest amplifies the effect of public opinion on policy as it raises the salience and visibility of an issue for legislators. As attention is scarce and politicians operate in a complex and volatile information environment (Baumgartner and Jones 2010), situations in which protest cues are congruent with citizens' priorities are more likely to induce legislative action. We know only of two studies that test this moderating role between public opinion and protest. Both Giugni (2007) and Agnone (2007), in what they respectively call a "joint-effect model" and "amplification model", mount evidence that protest indeed sensitizes legislators to public opinion.[2] We formalize this reinforcing link between public opinion and protest in our third hypothesis:

- **Amplification Hypothesis**: The effect of public issue priorities on attention change in legislative agendas rises with increasing protest.

[2]For a similar mechanism related to advocacy groups, see: Bevan and Rasmussen 2017; Rasmussen et al. 2017.

3 Data & methods

We are interested in the relationship between public priorities, protest and legislative agendas. Ideally, our data should ensure a measure of these concepts across a large sample of countries and time in order to estimate pooled time-series cross-section analyses. Unfortunately, comparable cross-national data for all three concepts is scarce. However, we managed to compile comparable data for Germany (1986-2011), Spain (1983-2011), the United Kingdom (1980-2011) and the United States (1974-1995).

Legislative agendas To measure legislative agendas, we rely on the data stemming from the collection efforts by the Comparative Agendas Project (CAP) (Baumgartner et al. 2009; Breunig and Schnatterer 2016; Chaqués-Bonafont et al. 2015; John et al. 2013; Jones and Baumgartner 2005). The CAP team collects legislative agendas across a total of 20 countries. Based on an exhaustive codebook, human coders allocate legislation to major topics as displayed in table 1.

Table 1: CAP issue codes

1. Macroeconomics	12. Law, Crime
2. Civil Rights	13. Social Welfare
3. Health	14. Housing
4. Agriculture	*15. Banking, Finance**
5. Labor & Employment	16. Defense + 19. International Affairs
6. Education	*17. Science**
7. Environment + 8. Energy	*18. Foreign Trade**
9. Immigration	*20. Government Operations**
*10. Transportation**	*21. Public Lands**

Note: * indicates major topic excluded from our analysis due to missing protest and/or public priorities data.

We rely on the Statutes of the American Congress and on UK Acts of Parliament to measure legislative agendas in the US[3] and the UK respectively (Bevan and Jennings 2014: 44).[4] For Germany and Spain we used data on legislative bills adopted by parliament (Breunig and Schnatterer 2016; Chaqués-Bonafont et al. 2015). While

[3]The data used here were originally collected by Frank R. Baumgartner and Bryan D. Jones, with the support of National Science Foundation grant numbers SBR 9320922 and 0111611, and are distributed through the Department of Government at the University of Texas at Austin. Neither NSF nor the original collectors of the data bear any responsibility for the analysis reported here.

[4]In all cases the coded time point is the date upon which a bill was signed into law.

the regulations of who introduces legislation and how it is adopted varies across our sample, in most countries legislation is crucially depending on support by the government.[5] Instead of using the counted number of laws for each country and time period, we calculated the share of legislation for each year and country – similar to previous research based on CAP data (Jennings and John 2009; Bevan and Jennings 2014).

Public agendas To measure public priorities – the set of policy issues to which the public attends (Jones and Baumgartner 2004) – we use surveys inquiring the 'most important problem/issue' (MIP/MII) facing the country (Cohen 1997; Canes-Wrone and Shotts 2004; Bevan and Jennings 2014). Although some discussion exists on the unclear definition in the most important problem/issue – i.e., whether an issue is really a problem, whether salience and importance are the same thing and whether variation in problem status can be correlated with importance over time (for details see: Wlezien 2005; Jennings and Wlezien 2011, 2015) – Jennings and Wlezien (2011: 554-555) find that MIP and MII series 'capture many of the same things, both at particular points in time and over time'. Hence, we use the aggregated MIP/MII responses to quantify public priorities. To guarantee comparability to the legislative agendas outlined above, we recoded all answers into the CAP's major topics described in table 1.[6] We then calculated the percentage of respondents listing a problem/issue the most important for each major CAP topic.

Protest agendas We merge three different datasets on collective action to retrieve the necessary information on political protest. First, we use the European Protest and Coercion dataset (EPCD) to measure protest in Germany, the UK and Spain from 1980 until 1995 (Francisco 1995, 1996, 2004). Unfortunately, the EPCD dataset does not cover the time period after 1995. Thus, starting in 1996 we relied on a dataset collected by Swen Hutter which largely continues the research undertaken by Kriesi and colleagues (Kriesi et al. 1995a, 2008; Hutter 2014). For the United Stated we employ the Dynamics of Collective Action (DCA) database for the entire period of observation (1974-1995) (McAdam and Su 2002; Earl et al. 2003).

[5] Regulations on who and how bills are adopted in the respective countries are outlined in detail in Breunig and Schnatterer (2016: 11) for Germany and in Spanish CAP Project for Spain.

[6] We adapted MIP series compiled by the Politbarometer for Germany, the Centro de Investigaciones Sociológicas (CIS) for Spain, Gallup and Ipsos-MORI for the UK and Gallup for the US. While we recoded the German and Spanish data ourselves, thankfully the CAP team made the UK and US data available online, under: `http://www.comparativeagendas.net`. Since data for the Gallup's MIP question in the UK are not available after 2001, Ipsos-MORI's MII data are also used; when overlapping, the two series are combined and averaged.

All three data-sets rely on information retrieved from newspaper articles to measure protest following a typical protest event analysis (see, e.g. Francisco 1995; Kriesi et al. 1995a).[7] This means that human coders were advised to code protest events extracted from newspaper articles.[8] Based on detailed codebooks, coders recorded comparable information and characteristics of protest events encompassing *who* protested, *where*, and *what* was the issue protested upon. The sources of information, however, vary across datasets.[9] Since the EPCD data also reports repressive acts by the government, and all three sources report a variety of events defined as protests, we focus in our analyses only on events ranging from peaceful forms such as rallies and public demonstrations to more radical actions such as blockades and occupations – again similar to previous research on protest (Giugni 2007: 60). This also means that our analyses only cover protests reported in the news. While this might sound like a limitation, protests not making the news are also heavily unlikely to affect policy-making.

Empirically, protest events come in different sizes and shapes – such as legal, illegal or violent protests. Partly due to this large set of characteristics, an accordance remains yet to be found on how to measure protest convincingly in comparative perspective. We decided to base our measure of protest on the frequency of protest events for each country and year. The frequency of protest is the most similar, simplest and complete measure of protest information across sources. Instead, measures such as the size of protest are highly unreliable: they may vary across newspapers for the exact same protest event and are oftentimes not reported, especially if the protest is small in numbers.[10] Lastly, the frequency of protest is highly correlated with the number of protesters, specifically if we rely on a yearly time-series.

[7] Newspapers covered by EPCD: Lexis-Nexis on full wire-service text with local newspapers included, (roughly 400 sources) exhaustive list can be found in the project codebook: `http://web.ku.edu/~ronfrand/data/`; by Kriesi/Hutter: one national quality newspaper per country; by DCA: New York Times.

[8] In the case of the EPCD data, the manual coding was assisted by machine-coding: "Most coders use the KEDS software to code the data. We then transfer the machine-assisted codes to an Excel spreadsheet, which is the primary coding document. We also code using Word and Excel simultaneously, using Excel's artificial intelligence to recognize each word in the column and to place suggestions while writing. Both of these methods increased the efficiency of coding" (EPCD Research Group: 1-2).

[9] A crucial difference is that the EPCD and the DCA databases used newspaper articles for each and every day for the time period of observation, while Kriesi et al. and Hutter relied only on the Monday edition of each newspaper. Thus, the number of protests reported in the datasets vary substantially.

[10] Overall the number of protesters is missing in 26.7 % of all protest events reported in our dataset (51.24 % DCA; 25.43 % Kriesi/Hutter; 3.48 % EPCD). We suspect that the number of missings in the EPCD is very small due to the fact that the codebook comes of with specific proceedings to code the number of protesters even if the media does not report the amount of mobilization.

In a first step, we recoded each protest event into the major topic codes provided by the CAP codebook.[11] Second, we calculated the share of protest for each topic, year and country for our dataset. Using the share of protest frequencies also addresses potential differences in the amount of protest events captured in the different datasets. This way we do not need to assume that datasets are equal in the amount of protests captured, but only in the respective share for each issue, which makes comparisons with public opinion straightforward.

In figure 1 we illustrate the structure of our data and provide a face validity check by using a smoothed local function (splines) to visually test if our data reports distinct and crucial breaks in the time trend.[12] If such breaks are visible and align with the time period for which we use a different data source for the European countries (1996) it would suggest that the datasets are reporting significantly different protest trends across time. As shown in figure 1, however, protests follow a comparably smooth trend between datasets. To be sure that coding differences between datasets are not worrying for our analyses, we also tested for breaks by estimating a time-series model differentiating the three data sources we are relying on. The major difference we are interested in is whether the continuation of Kriesi's work by Hutter reveals significant time trend differences. To do so we coded a dummy variable which is '1' for all time periods for which we use Swen Hutter's dataset. Again, we do not find a significant break in our time-series (β=5.9e^{-10}; P=0.114). This suggests that using percentages of protest frequency is a reliable measure across datasets.[13]

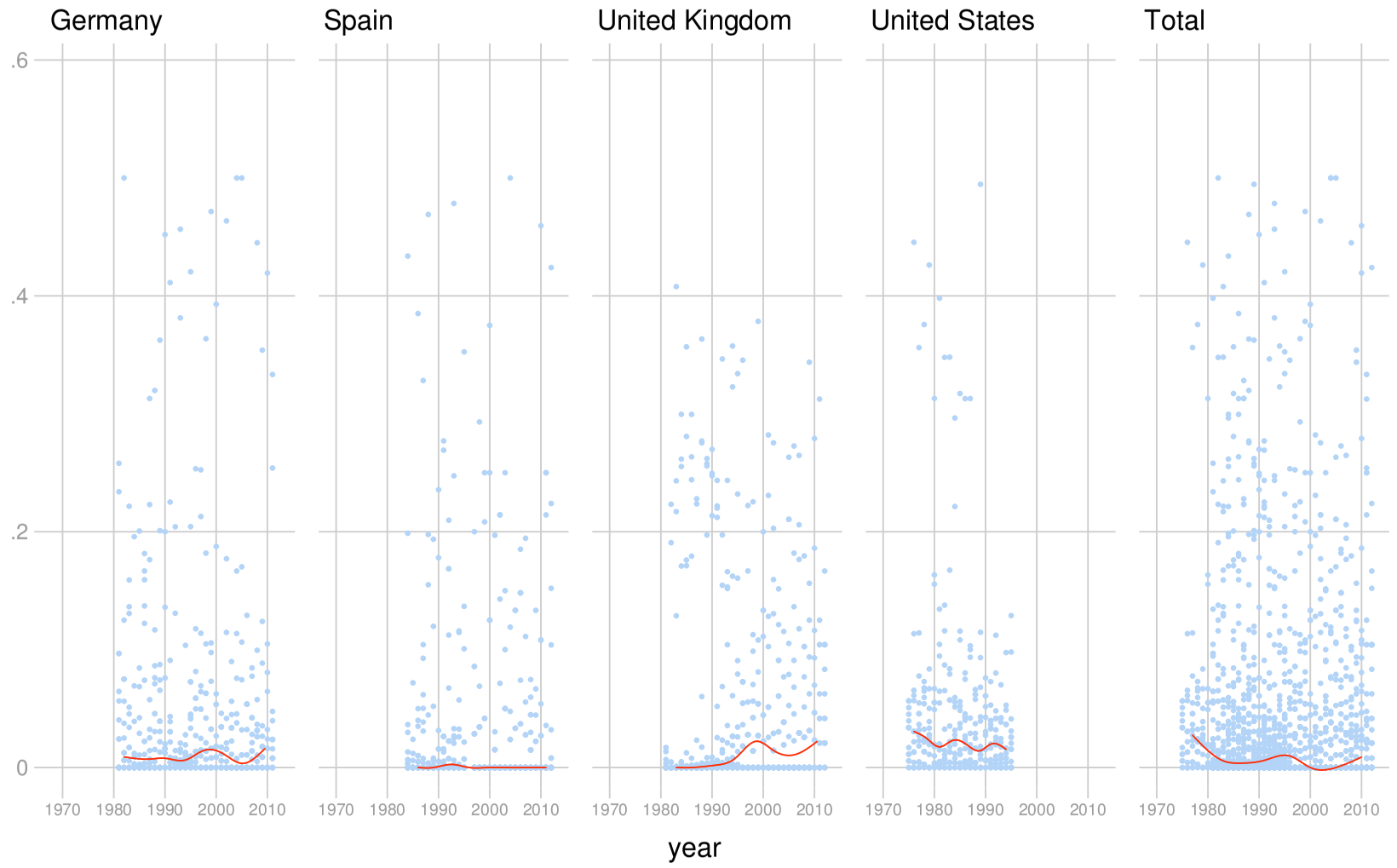
Figure 2, next, provides face validity for the quality of our data focusing on one crucial issue in our data: the environment. Notice that the scaling of the y-axes varies across countries to ensure readability of the US case. For instance, a clear peak of environmental protests in Germany is visible after the meltdown at the Chernobyl nuclear plant starting around 1986 and ending towards the 1990s. A comparable peak is visible for the US after the Three Mile Island accident in Dauphin County, Pennsylvania (1979). Finally, differences between public priorities and protesters' priorities are visible both across time and countries. While environmental concerns are of low salience both for the public and protesters' in the US, strong discrepancies are visible especially for Spain in the 2000s.

[11] We relied on the codebook for the US Agenda team to recode protest events.

[12] Notice that we omit outliers (values larger than 0.6) from figure 1.

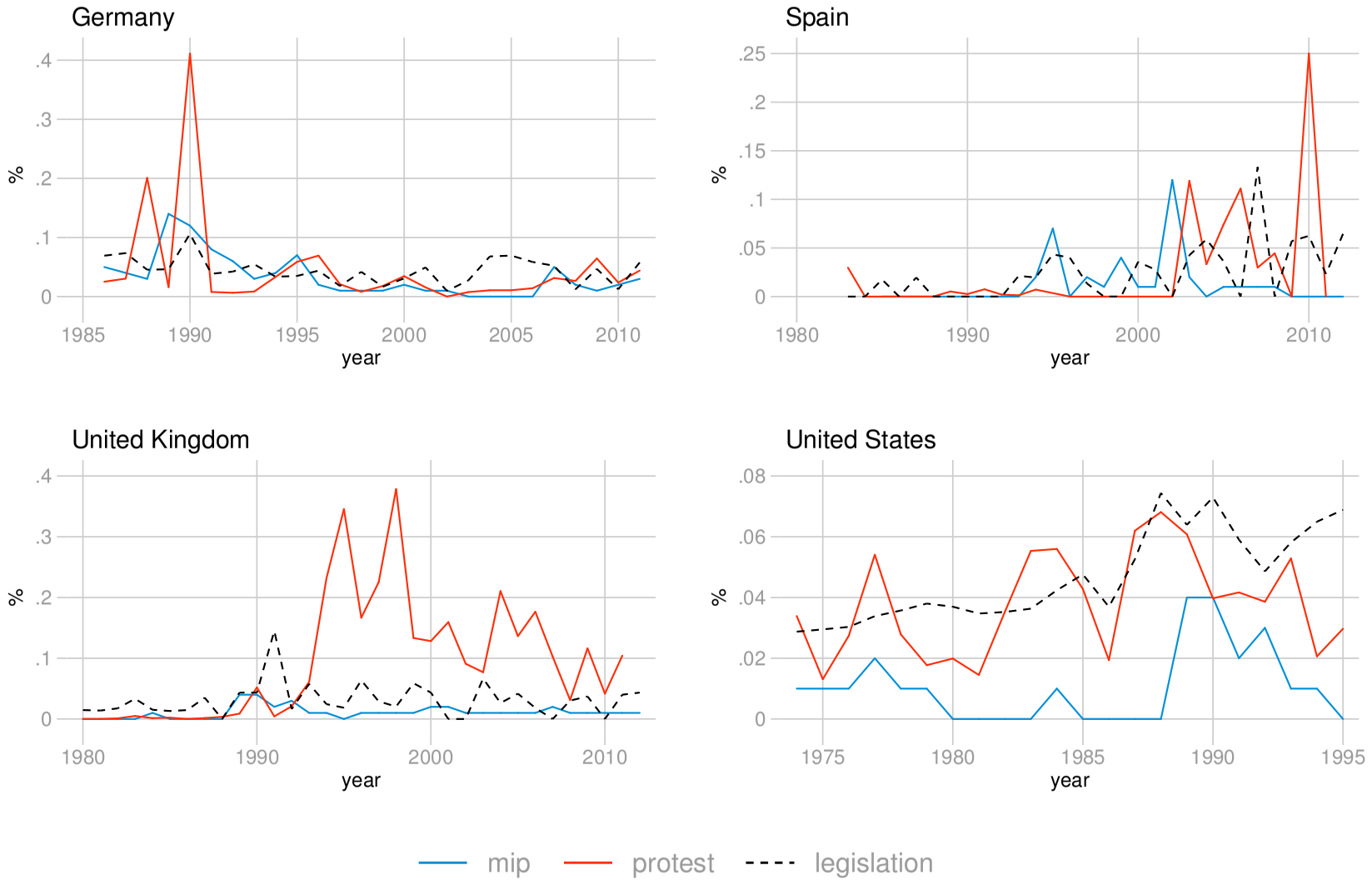
[13] To be sure the findings presented below are not driven by one country, we re-estimated our analyses separately for each country. Our findings are robust to these modeling strategies (see figure A.1 in the appendix).

Figure 1: Development of protest across time, per country



Source: Authors' own.

Figure 2: Development of protest & mip agenda for environmental concerns



Source: Authors' own.

3.1 Modeling strategy

Similar to previous research on dynamic agenda representation, we use an error correction model to analyze our data. More specifically, we estimate the following model in order to test our first two hypotheses (the Public Opinion Hypothesis and the Protest Hypothesis):

$$\begin{aligned}\Delta\text{Legislative Agenda}_{i,t} = \alpha_0 + \beta_1\text{Legislative Agenda}_{i,t-1} \\ +\beta_2\Delta\text{Public Priorities}_{i,t} + \beta_3\text{Public Priorities}_{i,t-1} \\ +\beta_4\Delta\text{Protest Priorities}_{i,t} + \beta_5\text{Protest Priorities}_{i,t-1} \\ +\zeta_{i,t} + \epsilon_{i,t}\end{aligned} \tag{1}$$

with the dependent variable being $\Delta LegislativeAgenda_t$, which denotes change in issue attention in legislative agendas between time t and time $t-1$, α the intercept, ζ a set of control variables outlined below, and ϵ the error term. Since we do not have strong theoretical reasons for restricting our model and we are interested in testing both short- and long-term effects of public priorities and protest, we follow De Boef and Keele's (2008) advice and estimate an unrestricted model. Hence, $\Delta PublicPriorities_t$ and $\Delta ProtestPriorities_t$ denote the change in public opinion priorities and protest frequency between time t and time $t-1$, respectively, whereas $PublicPriorities_{t-1}$ and $ProtestPriorities_{t-1}$ denote previous levels of public priorities and protest frequency, respectively. The coefficient on the variable $LegislativeAgenda_{t-1}$ denotes policy makers' long-term attention and evaluates whether policy makers that increased legislation on a given issue in the previous time period tend to legislate less on that issue in the current time period.

If the Public Opinion Hypothesis is supported, we should expect a positive and statistically significant coefficient on either the $\Delta PublicPriorities_t$ or the $PublicPriorities_{t-1}$ variables. If the Protest Hypothesis is supported, we should, instead, expect an insignificant coefficient on both the $\Delta ProtestPriorities_t$ and the $ProtestPriorities_{t-1}$ variables.

To evaluate the Amplification Hypothesis, we estimate a modified version of the model presented in equation 1. We test the conditional effect of public opinion and protest by including two interaction terms, one for the short- and one for the long-term effects: $\Delta PublicPriorities_t \times \Delta ProtestPriorities_t$ and $PublicPriorities_{t-1} \times ProtestPriorities_{t-1}$. If the hypothesis is supported, we should expect a positive and statistically significant

coefficient in either of the two variables. The model is presented below:

$$\begin{aligned}\Delta\text{Legislative Agenda}_{i,t} = \alpha_0 + \beta_1\text{Legislative Agenda}_{i,t-1} \\ +\beta_2\Delta\text{Public Priorities}_{i,t} + \beta_3\text{Public Priorities}_{i,t-1} \\ +\beta_4\Delta\text{Protest Priorities}_{i,t} + \beta_5\text{Protest Priorities}_{i,t-1} \\ +\beta_6\Delta\text{Public Priorities}_{i,t} \times \Delta\text{Protest Priorities}_{i,t} \\ +\beta_7\text{Public Priorities}_{i,t-1} \times \text{Protest Priorities}_{i,t-1} \\ +\zeta_{i,t} + \epsilon_{i,t}\end{aligned} \tag{2}$$

To efficiently use our data and to increase our statistical power, we rely on a stacked dataset. Our models are based on country-year observations, which are stacked by issue. Each country-year observation, hence, can appear 12 times, as there are 12 different issues in our dataset. Thus, the Gauss Markov assumptions of standard OLS regression analysis are likely to be violated. Indeed, autocorrelation tests reveal that the null hypothesis of no serial correlation needs to be rejected.[14] Further test-statistics provide evidence that the error terms are heteroscedastic and stationary.[15] The lagged dependent variable controls for autocorrelation. Given that the dataset is stacked by issues, we multi-way cluster our standard errors by country × years in the pooled analysis. In the country- and issue-specific models, we use robust standard errors due to the small clusters included in these analyses. Furthermore, as unobserved heterogeneity potentially infringes the results, we include country fixed effects in most models.

As it is common in time-series-cross-section analysis using lagged variables, it is difficult to judge how long an introduced lag should be. Law making is a cumbersome process, it can take time for legislators to prepare, discuss, draft, re-draft and to adopt legislation. Therefore, a lag of one year seems plausible and has been also used in most earlier research about legislative agendas and its adaptation to public opinion and protest (e.g., Bevan and Jennings 2014; Stimson et al. 1995; Olzak and Soule 2009).[16]

[14] A Wooldridge test for serial correlation in panel-data models is highly significant.

[15] A Fisher-type unit roots test is highly significant and a Cameron & Trivedi's decomposition of IM-test rejects the null hypothesis of homoskedasticity.

[16] However, in the robustness check section we discuss alternative lag structures.

4 Results

Table 2 reports the findings from these model specifications. Column 1 presents our baseline model using country fixed effects, where the direct effect of public priorities and protest is tested. Column 2 interacts the short-term effect of protest and public priorities. Column 3 interacts the long-run effect of protest and public priorities. Column 4 presents the full model with both short-term and long-term interaction variables.

Before turning to effects pertaining to public priorities, note that the coefficient on the variable $LegislativeAgenda_{t-1}$ is negative and significant in all analyses, while the coefficient on the intercept is positive and significant, implying a "regression to the mean" in legislative attention. That is, when legislative attention was unusually high (low) during the previous time period, then attention tended to subsequently decline (increase) in the current period.[17]

In line with previous research, we find support for our Public Opinion Hypothesis, whereby we observe substantively meaningful effects of public priorities on legislative agendas. A one percentage point increase of public priorities is associated with a 6 % higher chance of drafting a corresponding legislation. This suggests that, in general, legislators in our countries are responsive to public priorities in the long-run. Similar to previous studies, we do not find a significant short-term effect of public opinion priorities on attention change in legislative agendas. We think that this finding is plausible, for the outcome we care about is legislation. As outlined above, adopting legislation can take time which plausibly rules out short-term responsive reactions by legislators to changes in public priorities.

Turning to our Protest Hypothesis, namely that protest does not have a direct effect on legislation, we again find support for our hypothesis. In our analyses we do not find a significant effect of protest on legislation. We estimate the protest effect to be negative, suggesting that, if anything, protest is negatively associated with attention change in legislative agendas. As described in some research above cited, protest is frequently perceived as a signal from protesting minorities and not perceived as a relevant source of information driving the legislative agenda. The coefficient on the protest variables is smaller in size than the one on public priorities and not statistically significant.

Finally, looking into columns 2 to 4 of table 2, we do not find any support for an amplification mechanisms. Both short-term and long-term interactions are positive and point in the direction of an amplification mechanism, but they are not significant on

[17] This interpretation holds when the error-correction term is negative and falls between 0 and -1, when equilibrium shocks are corrected at a gradual rate (Jennings and John 2009: 841-842).

Table 2: Does protest influence attention change in legislative agendas? No.

	Δ legislative agenda$_t$			
	(1)	(2)	(3)	(4)
legislative agenda$_{t-1}$	-0.325***	-0.325***	-0.326***	-0.326***
	(0.033)	(0.033)	(0.035)	(0.035)
Δ protest$_t$	-0.017	-0.015	-0.017	-0.016
	(0.021)	(0.022)	(0.021)	(0.022)
Δ public priorities$_t$	0.059	0.054	0.060	0.054
	(0.029)	(0.032)	(0.029)	(0.032)
protest$_{t-1}$	-0.009	-0.009	-0.013	-0.013
	(0.024)	(0.024)	(0.024)	(0.024)
public priorities$_{t-1}$	0.064*	0.063*	0.060*	0.059*
	(0.021)	(0.021)	(0.023)	(0.023)
Δ public priorities$_t$ $\times$ Δ protest$_t$		0.390		0.382
		(0.653)		(0.665)
protest$_{t-1}$ $\times$ public priorities$_{t-1}$			0.058	0.054
			(0.086)	(0.092)
constant	0.023	0.023	0.023	0.023
	(0.004)	(0.004)	(0.005)	(0.005)
country FE	✓	✓	✓	✓
R^2	0.157	0.158	0.157	0.158
$adj.R^2$	0.151	0.151	0.150	0.150
N	1111	1111	1111	1111

pooled: (12 topics $\times$ 117 country/years)
* $p < 0.05$, ** $p < 0.01$, *** $p < 0.001$

conventional statistical thresholds.[18]

In short, our findings suggest that legislative agendas respond to public agendas but not protest agendas and we also find no evidence for an amplification effect of protest and public opinion.

4.1 Robustness Checks

Alternative model specifications We estimated several additional analyses reported in the Online Appendix to account for the robustness of our findings. First, dynamic representation theories show that representation can also be indirect, in that voters tend to elect governments who reflect their priorities (e.g., Bevan and Jennings 2014). Hence, it might be that policy makers stick to their longstanding issue commitments and policy priorities instead of responding directly to public concerns between election. We account

[18]Notice that we also estimated further combinations of interactions between the short and long running effects of protest and public priorities without finding any significant conditioning effect by protest on public priorities. E.g. protest$_{t-1}$$\times\Delta$ public priorities$_t$: β=-0.40; P=0.113. Δprotest$_t$$\times$ public priorities$_{t-1}$: β=-0.30; P=0.008. Even though this latter effect is statistically significant on conventional levels, we do believe that this might be a finding due to chance: we did not theorize upon such an effect and the effect is not in the expected direction.

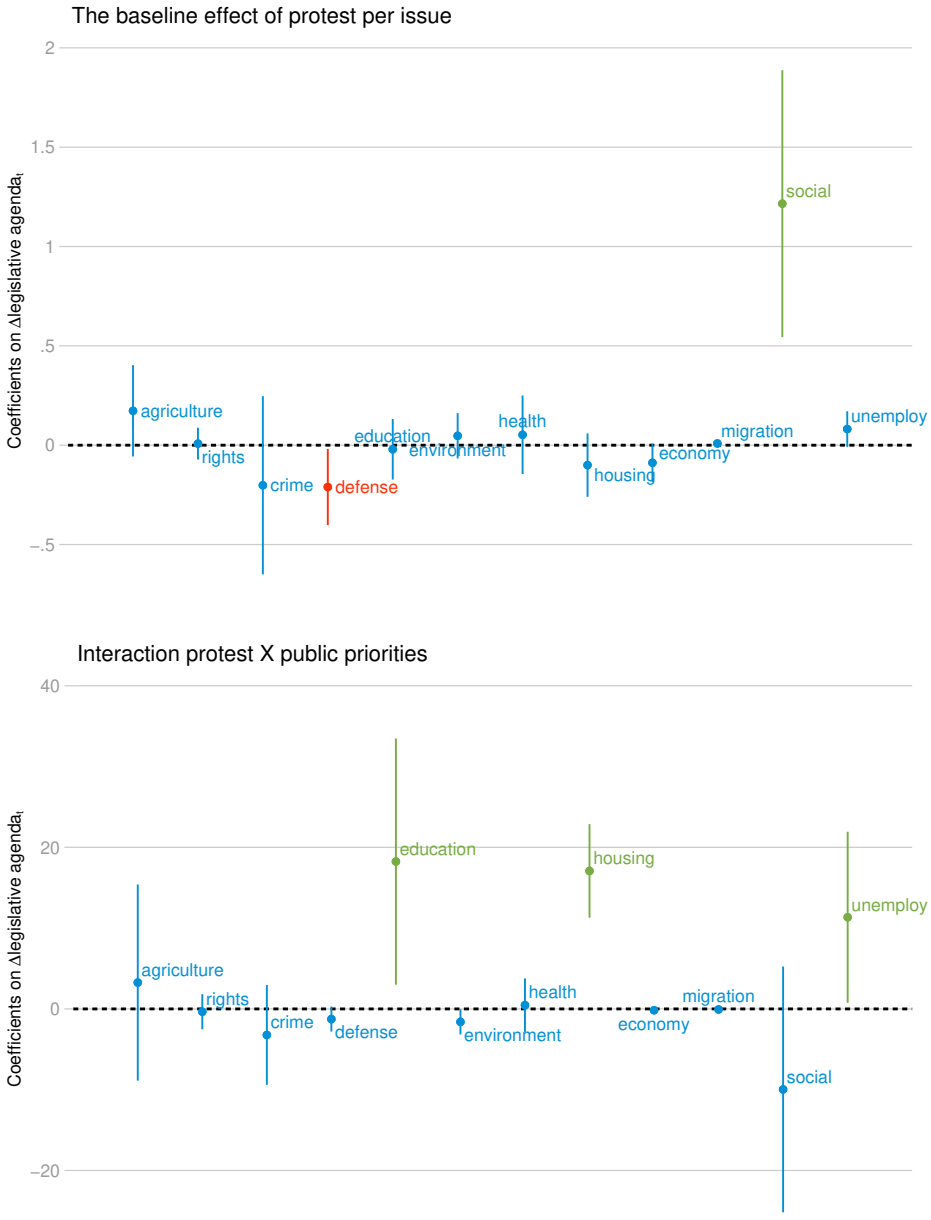
for this possibility and re-estimate our analyses while controlling for government partisanship using the mean left-right position of each government based on the RILE scale (Seki-Williams Government Partisanship) stemming from the Comparative Manifesto Data Project (CMP/MARPOR) (Budge et al. 2001) (Table A6). Second, since policy makers are forced to respond to real world problems such as economic downturns, we re-estimated our models while controlling for several economic factors such as unemployment, inflation and growth (Table A6). Third, as notoriously stated by Tsebelis (2002), since legislation should largely be a function of veto points within a given political system, we re-estimated our analyses while controlling for political constraints using Henisz' index (Henisz 2002) (Table A5). Fourth, it might be that legislative agendas respond to protest on issues that are highly salient compared to low-salience issues. To account for this possibility, we created a dummy variable coded as 1 if at least 50% of public opinion mentions the issue as their most important problem and 0 otherwise, and interact this majority priority variable with our protest variables (Table A5). Fifth, we tried to incorporate the level of protest into our analyses. Thus we re-estimated our analyses while controlling for two dummy variables measuring the size of protest – one capturing protest in a given year with more than 50,000 people, one capturing more than 100,000 per issue (Table A5). Sixth, although using one year lag accounting for the average length of the legislative process is preferable when analyzing the effect of public opinion and protest on legislation (e.g., Bevan and Jennings 2014; Stimson et al. 1995; Olzak and Soule 2009), we considered the idea that responsiveness might be faster or slower. Thus we re-estimated our analyses using biannual (Table A3) and biennial (Table A4) data. All these analyses keep supporting our substantive conclusions.

Analyzing cross-issue variation To better understand our findings, we re-estimated our models at the issue level. It might be the case that the association between protest and legislative agendas varies across issues with some issue domains being more plausibly affected by protest than others. Figure 3 reports the coefficients stemming from issue-based models using the same estimation strategy as outlined above.[19] The upper panel reports the direct effect of protest on legislative agendas. The bottom panel reports the long running interactions between protest and public priorities.

The upper panel supports our previous results of no direct effect of protest on change in legislative agendas for most issues covered in our data. The only issue reporting a

[19]Note that in these analyses we no longer cluster on the issue level but use robust standard errors for countries.

Figure 3: Does the effect of protest vary across issues? Yes.

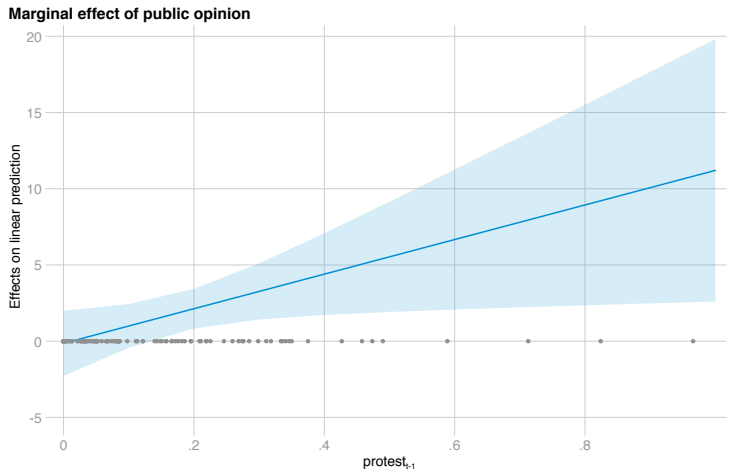


Source: Authors' own.
Note: Reported a coefficients from an error correction model using robust standard errors surrounded by 95 % confidence intervals.

positive association is social welfare. Here, protest seems to be an important source of information to develop legislation. We also find negative and significant but small effects of protest on legislation on issues on defense. However, these effects are substantively negligible and confidence in the point estimates is small as indicated by the large confidence intervals.

Turning our attention to the bottom panel, we find substantively meaningful and positive significant effects for the interaction between public priorities and protest for legislation on educational, housing and unemployed issues. Protest frequency on these obtrusive issues tends to upscale at various points in time in our data. Interestingly, however, questions of housing and education tend to rank low in public priorities. It seems that for these issues legislators nevertheless tend to listen more closely when protesters are active on the policy domain. This might be because politicians anticipate potential public reactions given the obtrusive nature of the issue when executing their legislative work. Finally, unemployment ranks high both for the public and protesters throughout various points in time. Here, we clearly observe an amplification mechanism as outlined by Agnone (2007).

Figure 5: Amplification effect of protest and public opinion for unemployment



Source: Authors' own.

Figure 5 reports the marginal effect of this interaction. It becomes visible that the amplification mechanism seems to be working throughout most values of political protest. The effect is substantive in size making legislation more likely by 11 %. Given

the high protest activity on the issue, particularly in Spain (throughout time), the United Kingdom (throughout time) and Germany (in the 1990s), an amplification effect seems most plausible on this issue instead of low-salience issues, such as the environment, which face infrequent protest activity throughout most periods we analyze.

5 Conclusion and discussion

Responsiveness to the issue priorities of the public is an essential part of democratic representation. According to democratic theory, what elected officials pay attention to should be in tune with what the public cares about. Yet how legislators respond to different kinds of public opinion signals is still an open question. We considered two types of public opinion signals: public issue priorities as measured through surveys and public issue priorities as expressed through protest. Given the rise of protest in Western democracies as a means to express public grievances, it makes sense to consider protest signals as relevant alternative public opinion cues (Meyer and Tarrow 1998; Norris 2002; Norris et al. 2005).

Bridging political science and social movement literature, we hypothesized that both public opinion and protest signals present legislators with different informational cues about what (a particular segment of) the public considers important. We, therefore, expected the political consequences of both to be different as well. Based on a novel assembled dataset, encompassing four countries and twelve issues over a twenty years' time span, we find public priorities as expressed through surveys to have a strong and positive impact on legislative attention. This finding is robust and confirms much of the previous research on dynamic representation. Protest, the alternative signal, has no significant effect on legislative attention. Moreover, the direction of the non-significant protest effect is negative. This confirms the classic image held by political scientists of protesters as beggars at the policy gate.

It is very likely, as we have put forward, that elected officials consider survey-based public issue priorities as more legitimate representations of the priorities of the general public. Priorities as expressed through protest, on the other hand, are considered as only the priorities of a particularly active segment, offering a biased view of the true public's priorities. Moreover, if protest is especially reactive, triggered by political disadvantages, the negative effect of protest is easily explained by the stickiness of the legislative agenda. Earlier policy stages might be more sensitive to protest cues, as previous research has shown, but once policy has changed and protest is triggered, chances of adaptation decrease.

The stronger effect of public opinion's priorities also might be a consequence of the clarity of the survey-based priority agenda. Our data shows that the survey-based public priority agenda is far less diffuse compared to the protest agenda. Public priorities are more clearly articulated through surveys, with some issues capturing the lion's share of attention and many issues staying well below the radar. In case of protest, many different issues capture only a moderate share of the agenda. This makes it much easier for elected officials to read, interpret and respond to the general public opinion agenda.

Protest signals are not entirely without legislative consequences, however. Contrary to the effect of public priorities, the effect of protest proves to be extremely contingent. This finding resonates with accounts of sociologists, who argue that protest especially can make a difference when staged in the right context. We find that only protest tackling social welfare issues has a direct and positive effect on change in legislative agendas. And, we find that protest amplifies the effect of public priorities – but only for the issues of housing, education and unemployment. These findings nicely put the impact of protest into perspective and illustrate that different public opinion signals affect attention change very differently. Whereas legislators routinely respond to public priorities as expressed through polls, they ignore protest in most cases. Only in quite exceptional situations, protest can make a difference.

Our data does not allow us to exactly put our finger on *why* the impact of protest materializes only under these specific circumstances, yet allows us to speculate about such conditions, which might inspire future research. For instance, the direct positive effect of protest on social welfare legislation might be a consequence of protest being staged by strong sponsoring organizations. These organizations often occupy insider positions and are well embedded within civil society and the policy making machinery. The impressive mobilizing capacity of strong sponsors allows them to affect politics using protest via two routes. First, they can stage protest actions that succeed to make a powerful assertion of popular sovereignty. The mobilization of large masses of diverse people impresses policy makers interested in re-election. Alternatively, when these organizations do not get what they want, their mobilizing capacity also puts them in the position to disrupt – or even threaten to disrupt – public order, which, costs anticipated by politicians, increases the protesters' bargaining power.

When it comes to the amplification effects found, it is telling that most of these issues, just like welfare, are very much of a "bread and butter" nature. These are issues that strongly impact citizens' lives. They are all quite obtrusive and, if not solved or accommodated, might play out as central issues in upcoming elections. It is reasonable

for legislators to avoid further politicization of such issues, and drafting legislation might be one way to get the noisy protesters off the public radar before they cause even more harm.

In all, how different public opinion signals translate into legislation turns out to differ strongly across signals and presents researchers with a complex puzzle. The analyses presented in this paper made a significant step forward by, for the first time, integrating public issue priorities and protest priorities across issues and countries and associating these with legislative attention. Whereas adaptation of legislation to public priorities as measured through surveys appears to be the default, routine, business as usual procedure in politics, the impact of protest shows to be more a matter of the rare, exceptional shock to the system. Our analyses straightforwardly confirm decades of research on democratic linkage, and substantiate the use of public priorities through surveys as an essential control when estimating the effects of other signals. With respect to these other signals, like protest, future research will need to flesh out the contingencies of its impact.

We expect that a focus on both characteristics of the signal and elements of the context will help researchers to disentangle the complex knot of diverse democratic linkage across public opinion signals. For instance, the institutional characteristics of the political system might moderate the impact of both public opinion and protest on legislation. In fact, Vliegenthart et al. (2016) find that the impact of protest is moderated by features of the political system via media attention on parliamentary questions; with stronger impact of protest in majoritarian compared to consensus democracies. We believe future research should go in this direction to better investigate the interactive impact of protest by controlling for protest characteristics and institutional features.

Finally, another specification of our general model might alter the temporal structure of the responsiveness chain. Although evidence is slim, several studies suggest that the impact of protest on legislation might be sequential, as in a two-step process, with protest first raising the attention of the public, and public opinion subsequently affecting legislators (Burstein 1979; Giugni 2007; McAdam and Su 2002; Santoro 2002). Similar specifications can be thought of for the receiver side of public opinion signals as well: it might be that protest and public opinion signals, in general, or on some issues, fare much better with particular parties, or if certain parties are in power. Although these specifications probably apply to earlier stages of the legislative process, opposition parties might be more responsive to protest compared to governing parties or parties that are issue-owner might be more prone to react to protest on that issue compared to others (Hutter and Vliegenthart 2016). A crucial finding in the recent wave of political impact

studies of protest is that exactly the presence of so-called "elite allies" in power is crucial for the success of protest. This is a difficult task that future research will have to deal with, as it implies classifying issue ownership across a large number of parties, issues and time.

In sum, whereas adaptation of legislation to public priorities as measured through surveys is characterized by a rich and extensive research tradition, this is far less the case for the protest-policy link, especially in comparative perspective and across issues. Studying the adaptation of legislation to different public opinion signals, and scrutinizing the contingencies of these associations opens promising avenues for future research. We leave it to future work to further investigate the above contingencies of the opinion-protest-policy nexus, and we hope that our first step encourages others to take up these challenges as well.

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A Appendix

A.1 Descriptive data overview

Table A.1: Summary statistics

Variable	Mean	Std. Dev.	N
Δlegislative agenda$_t$	0	0.065	1356
Legislative agenda$_{t-1}$	0.083	0.086	1356
Δprotest$_t$	0	0.114	1308
Δpublic priorities$_t$	0	0.045	1156
Protest$_{t-1}$	0.083	0.143	1344
Public priorities$_{t-1}$	0.066	0.132	1178

Table A.2: Summary statistics of protest across issues

CAP issue	Mean	Std. Dev.
Macroeconomics	0.129	0.182
Civil rights	0.146	0.193
Health	0.017	0.036
Agriculture	0.014	0.034
Unemployment	0.143	0.173
Education	0.041	0.083
Environment	0.069	0.1
Migration	0.148	0.194
Crime	0.058	0.096
Social welfare	0.058	0.096
Housing	0.012	0.031
Defense	0.215	0.156
N	112	

A.2 Robustness

Table A.3: OLS estimates, biannual data

	Δ legislative agenda$_t$			
	(1)	(2)	(3)	(4)
legislative agenda$_{t-1}$	-0.621***	-0.621***	-0.623***	-0.623***
	(0.063)	(0.063)	(0.064)	(0.064)
Δ protest$_t$	0.028	0.028	0.028	0.028
	(0.025)	(0.025)	(0.024)	(0.024)
Δ public priorities$_t$	0.030	0.018	0.032	0.023
	(0.096)	(0.100)	(0.096)	(0.102)
protest$_{t-1}$	-0.002	-0.003	-0.018	-0.017
	(0.041)	(0.040)	(0.038)	(0.039)
public priorities$_{t-1}$	0.142***	0.141***	0.126***	0.127***
	(0.021)	(0.021)	(0.021)	(0.022)
Δ public priorities$_t$ $\times$ Δ protest$_t$		0.584		0.466
		(0.718)		(0.730)
protest$_{t-1}$ $\times$ public priorities$_{t-1}$			0.184*	0.167*
			(0.073)	(0.074)
Constant	0.041***	0.041***	0.042***	0.042***
	(0.009)	(0.009)	(0.009)	(0.009)
country FE	✓	✓	✓	✓
R^2	0.308	0.309	0.309	0.310
$adj.R^2$	0.306	0.306	0.307	0.307
N	2258	2258	2258	2258

pooled: (12 topics $\times$ 117 country/years)

* $p < 0.05$, ** $p < 0.01$, *** $p < 0.001$

Table A.4: OLS estimates, biennial data

	Δ legislative agenda$_t$			
	(1)	(2)	(3)	(4)
legislative agenda$_{t-1}$	0.349***	0.350***	0.348***	0.350***
	(0.042)	(0.040)	(0.041)	(0.039)
Δ protest$_t$	-0.005	-0.004	-0.005	-0.004
	(0.019)	(0.018)	(0.020)	(0.019)
Δ public priorities$_t$	-0.003	0.001	-0.003	0.001
	(0.031)	(0.030)	(0.031)	(0.030)
protest$_{t-1}$	0.014	0.012	0.011	0.010
	(0.019)	(0.019)	(0.023)	(0.023)
public priorities$_{t-1}$	-0.091**	-0.092**	-0.094*	-0.093*
	(0.023)	(0.023)	(0.033)	(0.034)
Δ public priorities$_t$ $\times$ Δ protest$_t$		0.263		0.261
		(0.335)		(0.345)
protest$_{t-1}$ $\times$ public priorities$_{t-1}$			0.035	0.023
			(0.148)	(0.166)
Constant	-0.023***	-0.023***	-0.023***	-0.023***
	(0.005)	(0.005)	(0.004)	(0.004)
country FE	✓	✓	✓	✓
R^2	0.060	0.061	0.060	0.061
$adj.R^2$	0.052	0.052	0.052	0.052
N	1042	1042	1042	1042

pooled: (12 topics $\times$ 117 country/years)

* $p < 0.05$, ** $p < 0.01$, *** $p < 0.001$

Table A.5: OLS estimates, robustness tests

	Δ legislative agenda$_t$					
	(1)	(2)	(3)	(4)	(5)	(6)
legislative agenda$_{t-1}$	-0.323***	-0.324***	-0.334***	-0.345***	-0.328***	-0.326***
	(0.060)	(0.043)	(0.036)	(0.036)	(0.035)	(0.036)
Δ protest$_t$	-0.018	-0.016	-0.004	-0.019		
	(0.394)	(0.029)	(0.018)	(0.022)		
political constraints$_t$	-0.009	0.011				
	(0.018)	(0.022)				
Δ protest$_t$ $\times$ political constraints$_t$	0.002					
	(0.481)					
Δ public priorities$_t$	0.059	0.060	0.068	0.068	0.052	0.053
	(0.028)	(0.029)	(0.032)	(0.032)	(0.032)	(0.033)
protest$_{t-1}$	-0.008	0.184	-0.014	-0.016		
	(0.028)	(0.192)	(0.022)	(0.022)		
public priorities$_{t-1}$	0.063	0.062	0.125**	0.131***	0.061*	0.066**
	(0.034)	(0.031)	(0.033)	(0.028)	(0.020)	(0.016)
protest$_{t-1}$ $\times$ political constraints$_t$		-0.236				
		(0.247)				
majority priority$_{t-1}$			-0.057*	-0.063**		
			(0.021)	(0.018)		
majority priority$_{t-1}$ $\times$ Δ protest$_t$			-0.254**			
			(0.073)			
majority priority$_{t-1}$ $\times$ protest$_{t-1}$				-0.019		
				(0.028)		
protest$_{\geq 50'000}$					0.001	-0.003
					(0.007)	(0.006)
protest$_{\geq 100'000}$					0.006	0.014
					(0.008)	(0.007)
protest$_{\geq 50'000}$ $\times$ public priorities$_{t-1}$						0.067
						(0.039)
protest$_{\geq 100'000}$ $\times$ public priorities$_{t-1}$						-0.105
						(0.051)
constant	0.030	0.014	0.022***	0.022***	0.021***	0.021***
	(0.016)	(0.021)	(0.004)	(0.004)	(0.004)	(0.004)
country FE			✓	✓	✓	✓
R^2	0.156	0.157	0.181	0.171	0.158	0.160
$adj.R^2$	0.151	0.152	0.173	0.163	0.152	0.152
N	1111	1111	1111	1111	1111	1111

pooled: (12 topics $\times$ 117 country/years)
* $p < 0.05$, ** $p < 0.01$, *** $p < 0.001$

Table A.6: OLS estimates, robustness tests with controls

	Δ legislative agenda$_t$			
	(1)	(2)	(3)	(4)
legislative agenda$_{t-1}$	-0.326***	-0.326***	-0.328***	-0.327***
	(0.042)	(0.042)	(0.043)	(0.044)
Δ protest$_t$	-0.016	-0.015	-0.017	-0.016
	(0.023)	(0.024)	(0.023)	(0.024)
Δ public priorities$_t$	0.058	0.053	0.059	0.054
	(0.029)	(0.033)	(0.029)	(0.033)
protest$_{t-1}$	-0.008	-0.009	-0.014	-0.014
	(0.025)	(0.025)	(0.025)	(0.025)
public priorities$_{t-1}$	0.064*	0.064*	0.060*	0.059
	(0.024)	(0.024)	(0.027)	(0.027)
government RILE$_{t-1}$	0.000	0.000	0.000	0.000
	(0.000)	(0.000)	(0.000)	(0.000)
Δ government RILE$_t$	0.000	0.000	-0.000	-0.000
	(0.000)	(0.000)	(0.000)	(0.000)
GDP growth	0.000	0.000	0.000	0.000
	(0.001)	(0.001)	(0.001)	(0.001)
GDP growth$_{t-1}$	-0.000	-0.000	-0.000	-0.000
	(0.000)	(0.000)	(0.000)	(0.001)
unemployment$_{t-1}$	-0.001	-0.001	-0.001	-0.001
	(0.001)	(0.001)	(0.001)	(0.001)
Δ unemployment$_t$	0.000	0.000	0.000	0.000
	(0.002)	(0.002)	(0.002)	(0.002)
inflation$_{t-1}$	-0.000	-0.000	-0.000	-0.000
	(0.000)	(0.000)	(0.000)	(0.000)
Δ inflation$_t$	-0.000	-0.000	-0.000	-0.000
	(0.001)	(0.001)	(0.001)	(0.001)
Δ public priorities$_t$ $\times$ Δ protest$_t$		0.374		0.365
		(0.689)		(0.703)
protest$_{t-1}$ $\times$ public priorities$_{t-1}$			0.063	0.059
			(0.109)	(0.115)
Constant	0.028*	0.028*	0.029*	0.028*
	(0.009)	(0.009)	(0.011)	(0.011)
country FE	✓	✓	✓	✓
R^2	0.158	0.158	0.158	0.159
$adj.R^2$	0.145	0.145	0.145	0.145
N	1111	1111	1111	1111

pooled: (12 topics $\times$ 117 country/years)

* $p < 0.05$, ** $p < 0.01$, *** $p < 0.001$

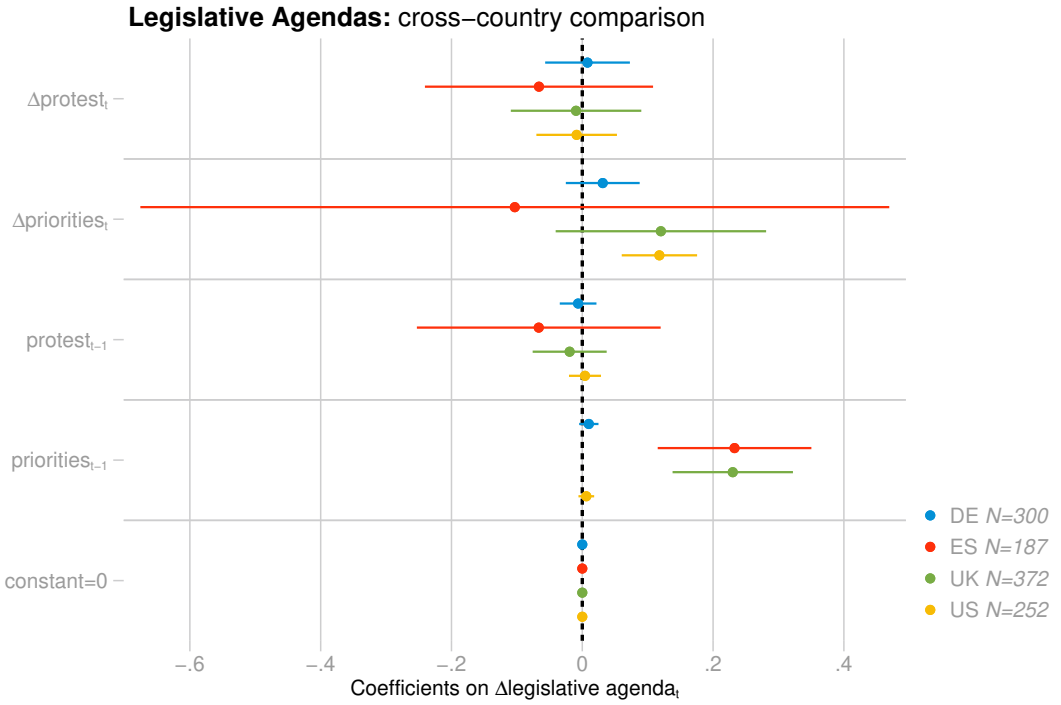
Table A.7: OLS estimates, models per country

	Δ legislative agenda$_t$			
	(1)	(2)	(3)	(4)
	Germany	Spain	United Kingdom	United States
legislative agenda$_{t-1}$	-0.133**	-0.608***	-0.498***	-0.050
	(0.034)	(0.070)	(0.081)	(0.032)
Δ protest$_t$	0.008	-0.066	-0.010	-0.009
	(0.029)	(0.078)	(0.045)	(0.028)
Δ public priorities$_t$	0.031	-0.103	0.120	0.118***
	(0.026)	(0.257)	(0.073)	(0.026)
protest$_{t-1}$	-0.006	-0.067	-0.019	0.004
	(0.013)	(0.084)	(0.026)	(0.011)
public priorities$_{t-1}$	0.010	0.233**	0.230***	0.006
	(0.007)	(0.053)	(0.042)	(0.005)
Constant	0.011***	0.031	0.027**	0.003
	(0.002)	(0.014)	(0.007)	(0.003)
constant	✓	✓	✓	✓
R^2	0.065	0.285	0.247	0.103
$adj.R^2$	0.049	0.265	0.236	0.085
N	300	187	372	252

pooled: (12 topics $\times$ 117 country/years)

* $p < 0.05$, ** $p < 0.01$, *** $p < 0.001$

Figure A.1: Does the effect of protest vary across countries? No.



Source: Authors' own.
Note: Reported a coefficients from an error correction model using robust standard errors surrounded by 95 % confidence intervals (full findings are reported in table A.7 on page 36 in the appendix).